case is, that H_1 lies in a part of the curve ascending to or descending from a higher summit. Then the ordinates on the one side of H_1 will be greater, and on the other less than H_1. But because higher summits are so extremely improbable, the first case will be the most probable, and if we choose an ordinate of given magnitude H_1 guided by haphazard in the curve, it will be not certain, but very probable, that the ordinate decreases if we go in either direction.

We will now assume, with Mr. Culverwell, a gas in a given state. If in this state H is greater than H (min.) it will be not certain, but very probable, that H decreases and finally reaches not exactly but very nearly the value H (min.), and the same is true at all subsequent instants of time. If in an intermediate state we reverse all velocities, we get an exceptional case, where H increases for a certain time and then decreases again. But the existence of such cases does not disprove our theorem. On the contrary, the theory of probability itself shows that the probability of such cases is not mathematically zero, only extremely small.

Hence Mr. Burbury is wrong, if he concedes that H increases in as many cases as it decreases, and Mr. Culverwell is also wrong, if he says that all that any proof can show is, that taking all values of dH/dt got from taking all the configurations which approach towards a permanent state, and all the configurations which recede from it, and then striking some average, dH/dt would be negative. On the contrary, we have shown the possibility that H may have a tendency to decrease, whether we pass to the former or to the latter configurations. What I proved in my papers is as follows: It is extremely probable that H is very near to its minimum value; if it is greater, it may increase or decrease, but the probability that it decreases is always greater. Thus, if I obtain a certain value for dH/dt, this result does not hold for every time-element dt, but is only an average value. But the greater the number of molecules, the smaller is the time-interval dt for which the result holds good.

I will not here repeat the proofs given in my papers; I will only show that just the same takes place in the much simpler case of dice. We will make an indefinitely long series of throws with a die. Let A_1 be the number of times of throwing the number 1, among the first $6n$ throws, A_2 the number of times of throwing 1, among all the throws between the second and the $(6n+1)$th inclusive, and so on. Let us construct a series of points in a plane, the successive abscissæ of which are

$$0, \frac{1}{n}, \frac{2}{n}, \frac{3}{n}, \ldots,$$

the ordinates of which are

$$y_1 = \left(\frac{A_1}{n} - 1\right)^2, y_2 = \left(\frac{A_2}{n} - 1\right)^2 \ldots;$$

let us call this series of points the "P-curve." If n is a large number, the greater proportion of the ordinates of this new curve will be very small. But the P-curve (like the aforementioned H-curve) has summits which are higher than the ordinary course of the curve. Let us now consider all the points of the P-curve, whose ordinates are exactly = 1. We will call these points "the points B." Since for each point $y = (A/n - 1)^2$, therefore for the points B we have $A = 2n$; these points mark, therefore, the case where, by chance, we have thrown the number 1 in $2n$ out of $6n$ throws. If n is at all large, that is extremely improbable, but never absolutely impossible. Let v be a number much smaller than n, and let us go forward from the abscissa of each point B through a distance $= 6v/n$ in the direction of x positive. We shall probably meet a point, the ordinate of which < 1. The probability that we meet an ordinate > 1 is extremely small, but not zero. By reasoning in the same manner as Mr. Culverwell, we might believe that if we go backward (*i.e.* in the direction of x negative) from the abscissa of each point B through a distance $= 6v/n$, it would be probable that we should meet ordinates > 1. But this inference is not correct. Whether we go in the positive or in the negative direction the ordinates will probably decrease.

We can even calculate the probable diminution of y. We have seen that for every point B we have $A = 2n$ (*i.e.* $2n$ throws out of $6n$ turning up 1). If we move in the positive or negative direction along the axis of x through the distance $1/n$, we exclude one of the $6n$ throws, and we include a new one. When we move forward through the distance $6v/n$, we have excluded $6v$ of the original throws, and included $6v$ others. Among the excluded throws we have probably $2v$, among the included ones v throws of the number 1. Therefore the probable diminution of A is v, the probable diminution of y is $2v/n$ approximately. Because the variation of x was $6v/n$, we may write

$$\frac{dy}{dx} = -\frac{1}{3}.$$

But this is not an ordinary differential coefficient. It is only the average ratio of the increase of y to the corresponding increase of x for all points, whose ordinates are = 1. The P-curve belongs to the large class of curves which have nowhere a uniquely defined tangent. Even at the top of each summit the tangent is not parallel to the x-axis, but is undefined. In other words, the chord joining two points on the curve does not tend towards a definite limiting position when one of the two points approaches and ultimately coincides with the other.[1] The same applies to the H-curve in the Theory of Gases. If I find a certain negative value for dH/dt, that does not define the tangent of the curve in the ordinary sense, but it is only an average value.

§ 3. Mr. Culverwell says that my theorem cannot be true because if it were true every atom of the universe would have the same average *vis viva*, and all energy would be dissipated. I find, on the contrary, that this argument only tends to confirm my theorem, which requires only that in the course of time the universe must tend to a state where the average *vis viva* of every atom is the same and all energy is dissipated, and that is indeed the case. But if we ask why this state is not yet reached, we again come to a "Salisburian mystery."

I will conclude this paper with an idea of my old assistant, Dr. Schuetz.

We assume that the whole universe is, and rests for ever, in thermal equilibrium. The probability that one (only one) part of the universe is in a certain state, is the smaller the further this state is from thermal equilibrium; but this probability is greater, the greater is the universe itself. If we assume the universe great enough, we can make the probability of one relatively small part being in any given state (however far from the state of thermal equilibrium), as great as we please. We can also make the probability great that, though the whole universe is in thermal equilibrium, our world is in its present state. It may be said that the world is so far from thermal equilibrium that we cannot imagine the improbability of such a state. But can we imagine, on the other side, how small a part of the whole universe this world is? Assuming the universe great enough, the probability that such a small part of it as our world should be in its present state, is no longer small.

If this assumption were correct, our world would return more and more to thermal equilibrium; but because the whole universe is so great, it might be probable that at some future time some other world might deviate as far from thermal equilibrium as our world does at present. Then the afore-mentioned H-curve would form a representation of what takes place in the universe. The summits of the curve would represent the worlds where visible motion and life exist.

LUDWIG BOLTZMANN.

Imperial University of Vienna.

Oysters and Typhoid.

WITH reference to the article "Oysters and Typhoid," which appeared in your last issue, it may interest your readers to know that De Giaxa investigated some years ago the behaviour of the typhoid bacillus in sea-water, both in its natural and sterilised condition. He found that in ordinary sea-water the typhoid bacillus suffered very considerably in the competition with the numerous other water bacteria present, but it was still identified on the ninth day after it was first introduced. In sea-water in which all other bacteria had been destroyed, the typhoid bacillus was detected in very appreciable numbers on the twenty-fifth day. More recently, however, the existence of typhoid bacilli in sterilised sea-water has been examined by Cassedebat, and his results are not in accord with those obtained by Giaxa. Cassedebat found that whilst many pathogenic

---

[1] See Ulisse Dini, "Grundlagen für eine Theorie der Functionen einer reellen Veränderlichen" (Teubner, 1892, § 126), or Weierstrass, *Journal für die Mathematik*, Band 79, p. 29.

© 1895 Nature Publishing Group

bacteria, amongst which were those of anthrax and cholera, lived for several days, in the case of the former twenty-one to twenty-four days, and in that of the latter thirty-five days and even longer, typhoid bacilli were destroyed already in the course of forty-eight hours in sterilised sea-water. These results tend to confirm those obtained by Prof. Percy Frankland, who, in his last report to the Royal Society Water Research Committee, states that the addition of 1 and 3 per cent. of common salt to ordinary Thames water into which typhoid bacilli were introduced, acted very prejudicially on the latter, although it stimulated the multiplication of many forms of water bacteria present in a most astonishing manner. Similar results were obtained with salted but sterilised Thames water infected with typhoid bacilli, and not only did the latter disappear very rapidly, but the typhoid colonies which were obtained on plate-cultures from this salt water exhibited in some cases a very abnormal appearance, attributable to the degeneration of the typhoid bacilli under these conditions, for on passing such colonies through a further process of plate-culture, they returned to their ordinary type.

Giaxa also experimented with fish, introducing pathogenic microbes *per os*. Unfortunately, he did not select the typhoid bacillus for these experiments, but took instead cultures of cholera and anthrax bacilli. He found that in both cases these micro-organisms were entirely destroyed in the course of a few hours. Experiments were also made with oysters and some varieties of mussel-fish. A small hole was bored in the shell, and vigorous broth cultures of anthrax and cholera bacilli were respectively introduced, after which the aperture was carefully closed with sealing-wax, and whilst some of the molluscs were replaced in sea-water, others were kept in glass dishes without any water. The latter lived for two days under these conditions. Six hours after the injection had taken place, and again at the end of twenty-four and forty-eight hours, the sealing-wax was removed and a small quantity of the fluid within the shell was taken out and plate-cultures made. In the majority of the experiments these pathogenic microbes had completely disappeared in six hours, whilst in only two instances were they detected in small numbers at the end of twenty-four hours, and in no case were they identified after forty-eight hours. These results were irrespective of whether the mollusc was kept in or out of water. In these investigations, therefore, there would appear to be no evidence that these pathogenic microbes (cholera and anthrax) were capable of being transmitted by means of these shell-fish. So far as I am aware, no one has followed up these interesting experiments, and in view of the serious allegations which have lately been made against oysters as transmitters of typhoid fever, it is a subject which might well claim re-investigation.

February 13. G. C. FRANKLAND.

The Occurrence of very Cold Days.

IT is usual to estimate cold by minima of temperature. But we may also consider it as expressed in maxima. A day may fairly be called "very cold" in which the maximum is not over freezing point. I propose to offer some account of the occurrence of such days at Greenwich in the fifty winter seasons (November to March) 1844–5 to 1893–4. (For brevity, I will designate each winter by the year in which it begins.)

These very cold days are not very frequent. In some winters there are none. The highest number is 27. The list is as follows:—

1844	13	1861	5	1878	18
45	—	62	—	79	10
46	11	63	4	80	12
47	4	64	5	81	—
48	2	65	—	82	2
49	7	66	12	83	—
50	—	67	6	84	1
51	—	68	1	85	3
52	—	69	8	86	5
53	4	70	10	87	5
54	15	71	1	88	2
55	4	72	1	89	1
56	3	73	3	90	27
57	2	74	5	91	5
58	1	75	4	92	9
59	6	76	—	93	5
60	9	77	—		
				Total	251

The total, 251, gives an average of 5·02 for each season. Ten of the winters had none of those days, and fifteen had numbers over the average.

The distribution in months was as follows:—

	November.	December.	January.	February.	March.
	6	96	111	34	4
Average	—	1·92	2·22	·68	—

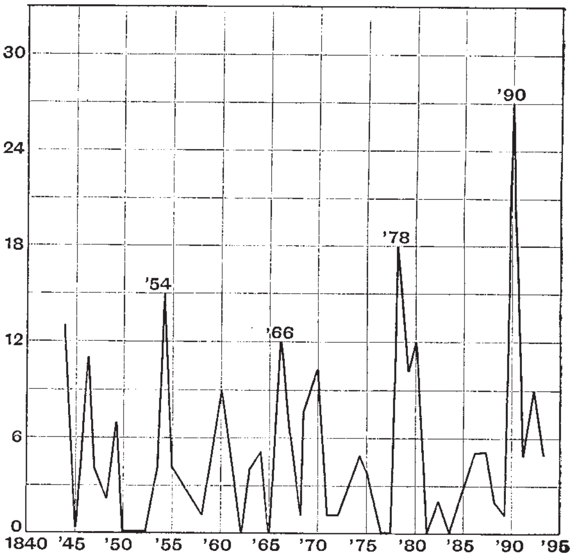
Such days are rare in November; and still more rare in March. The six cases in the former month were in 1849, 1858, 1861, 1887, and two in 1890; the earliest in the month being that in 1887 (16). The four cases in March were all in one year, 1845. January has most of such days (about 17 per cent. more than December).

The largest number in any one month was sixteen in December 1890; the next, twelve in January 1867 and January 1881.

We might perhaps note that relative maxima of the seasonal numbers occurred in 1854 (15), in 1866 (12), in 1878 (18), and in 1890 (27); giving three intervals of twelve years.

Measured by mean temperature of the three winter months, these winters were all severe (considerably under average) except 1866; in which, though January (1867) was very cold, December and February were both mild (the latter uncommonly so); thus the winter was above the average of mean temperature.

Days with Maxima not over 32° at Greenwich.



These very cold days most often come in groups (two or more in succession). The largest group was 10 in December '90; the next, 9 in January '81; then 6 in December '44, December '59, and January '93, &c.

Of the 251 days, 100 had maxima from 31° to 32°; 51 from 30° to 31°; 42 from 29° to 30°; 23 from 28° to 29°; 14 from 27° to 28°; and 21 under 27°, ranging down to 19°. The corresponding minima range from 29°·6 down to 7°·7, with greatest density about 22° to 24° (45 cases).

How are the maxima related to their respective minima? There is, of course, no rigorous proportionality; and we may often find high maxima with very low minima (*e.g.* December 29, '60, max. 31°·8, min. 10°·0), as also low maxima with relatively high minima (*e.g.* January 15, '50, max. 26°·9, min. 25°·9). On an average, however, high minima go with high maxima, and low with low. Take the following:

I selected the cases in which the maxima was under 27°; there are 21. Of the corresponding minima only four were *over* 20°. On the other hand, taking the last 21 (of 46) cases in which the maximum was over 31°·5, one finds only four minima *under* 20°. The average minimum in the former case was 16°·4; in the latter 24°·4. The average difference of maxima and minima was in both cases about 8°.

© 1895 Nature Publishing Group